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On stricture as the initial  
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CONDUCTED BY

E. L. YOUMANS.

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ON  
STRICTURE  
AS THE  
INITIAL CAUSE OF GLEET,  
WITH  
REMARKS ON THE URETHRAL CALIBRE,  
*BEING A REPLY TO THE PAPER OF DR. H. B. SANDS  
ON THE SAME SUBJECT.*

BY  
FESSENDEN N. OTIS, M.D.,  
CLINICAL PROFESSOR OF GENITO-URINARY DISEASES, COLLEGE OF PHYSICIANS AND  
SURGEONS, NEW YORK; SURGEON TO THE CHARITY HOSPITAL, ETC.

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ON STRICTURE AS THE INITIAL CAUSE  
OF GLEET, WITH REMARKS ON THE  
URETHRAL CALIBRE. BEING A REPLY  
TO THE PAPER OF DR. H. B. SANDS ON  
THE SAME SUBJECT.<sup>1</sup>

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PROF. HENRY B. SANDS opened his interesting and able paper "On Gleet, and especially in its Relations to Urethral Stricture," with these words: "*The humiliating confession must be made, that many important surgical problems remain unsolved.*" This was the statement of a fact, which, in his opinion, was especially applicable to the subject which he was about to discuss.

In so many words, then, he confessed that gleet, and especially in its relations with urethral stricture, was a problem for which he had, as yet, found no satisfactory solution. The object of the paper, as stated, was simply to excite discussion, and particularly with reference to views which had been advanced by me, in which I claimed to designate the true nature and cause of gleet, and the only effectual and radical cure for this acknowledged opprobrium of surgery.

Proceeding then to the definition of gleet, Prof. Sands referred to *idiopathic gleet*—gleet depending upon a *strumous diathesis*, *prostatic gleet*, *masturbators' gleet*, etc., and re-

<sup>1</sup> Published in the March number of the NEW YORK MEDICAL JOURNAL.

marked, that "we shall avoid much confusion, by giving to the word the restricted meaning ascribed to it by John Hunter and Sir Astley Cooper, and regard it as an *imperfect* or *chronic gonorrhœa*." Now, as this matter is presented, avowedly, for the purpose of discussing my peculiar views in regard to *gleet*, and its *relations* to urethral stricture, I shall most decidedly object to any such definition of *gleet*, as being, not simply imperfect, but, as conveying impressions, which, of necessity, will often lead to grave errors in the diagnosis and treatment of *gleet*.

I have stated it to be the rule, that all *gleet* depends upon *stricture*, not that, all *gleet* depends upon *gonorrhœa*. It seems to me, then, from my point of view, that, in order to consider the question of *gleet* intelligently, we must first discuss the nature and causes of *stricture*; having settled these points, the different varieties of *gleet* will be sufficiently indicated. In regard to the nature and causes of *gleet*, in the opinion of Prof. Sands, this disease is simply *catarrhal*.

1. He says: "Gleet depends upon a chronic inflammation of the urethral mucous membrane, either diffused over a greater part, or limited to spots, chiefly to the fossæ navicularis and bulb.

"2. Gleet depends upon inflammation of lacunæ opening into the urethra.

"3. Gleet depends upon *stricture of the urethra*."

Supported by these three postulates, Prof. Sands takes exception to my claim that "*gleet always depends upon stricture*."

A *careful*, not a *hypercritical*, examination of his position will, I believe, tend to simplify the assumed points of difference, between Prof. Sands and myself, in regard to the causes of *gleet*, very materially.

I do not deny that it is a chronic inflammation; nor that it is sometimes found to be diffused over a great part of the urethra; nor that it is sometimes confined to spots; nor yet that it may occupy the continuous lining of the lacunæ and mucous follicles. In the present discussion, however, the locality and pathological results of *gleet* are not so much at issue, as the *cause* of the *continuance* of those conditions upon



which the muco-purulent discharge, which we call *gleet*, depends.

I have so often seen *diffused* and *localized* inflammatory conditions of the urethral mucous membrane associated with urethral stricture, and have so constantly seen them disappear, upon the division of the stricture, that I do not hesitate to affirm my conviction that *all granular spots in the urethra* are the result of retention of acrid urine, behind strictures more or less salient; and that the most favorable condition to induce implication of the lacunæ magna and the deeper sinuses and follicles is the presence of an anterior stricture.

The term *gleet* is used by Prof. Sands as indicating an imperfect or chronic gonorrhœa. Now, gonorrhœa is a self-limited disease; an active inflammation, produced by contagion, which continues, according to our best authorities, for three or four weeks, under the most judicious treatment (and I may here add, whether treatment is had or not—for it is the rule that, in favoring physical conditions, it gets well, in about that time, with no treatment whatever). Prof. Sands says: "A gonorrhœal discharge, after it has continued from one to four weeks, *almost always* diminishes in quantity, becomes thinner and less opaque, and, in favorable cases, disappears altogether." Again he says, "*Not unfrequently*, the discharge *does not* disappear, and *may continue indefinitely*." The difference, then, between gonorrhœa and "chronic or imperfect gonorrhœa" or *gleet* is, that the one gets well and the other continues indefinitely. In order to ascertain the reason of this continuance of a gonorrhœa—to find what constitutes the *punctum malum*—the essential difference between the favorable and the unfavorable cases, Prof. Sands gives a *résumé* of the results of the pathological researches of Sir Astley Cooper, Rokitansky, and Sir Henry Thompson. In some cases, unusual vascularity was found at the fossæ navicularis; in others, general tumefaction of mucous membrane; enlargement of follicles, relaxation of sinuses, etc., and yet in other cases "*no abnormal appearance could be detected*." The summing up, then, of *these* researches seems to shed no light upon the *cause* of gleet; it simply presents the results of long-continued inflammation, of a low grade, in certain cases; while



the fact that, in other cases, *nothing abnormal* was found, is a sufficient commentary on the value of this method of ascertaining the cause of gleet. The pathological *résumé*, then, simply shows, that the cause of gleet was not determined by any *post-mortem* examination. Prof. Sands then cites the results of observations during life. First, "soreness and smarting," he says, "may exist and mean nothing;" "frequent desire to micturate *may* mean the presence of a morbid process in the urethra; sensation of straining *may* point to urethral trouble, but gleet *may exist* and *persist*, unaccompanied by any morbid sensation." Circumscribed granular spots *may* exist, and be revealed by the endoscope, but cannot be regarded as the *sole* cause of gleet.

Observations during life, then, do not afford any definite information as to the *cause* of gleet.

The results of the action of remedies are then invoked to discover the cause of gleet. "Sometimes," says Prof. Sands, "its removal is favored by a spare, and at others by a generous diet. Sea-bathing and tonic food and medicines have cured many a gleet, that has resisted the ordinary specific remedies for the disease. In a strumous, gouty, or rheumatic diathesis, appropriate constitutional treatment may become necessary. "Copaiba and cubebs," he says, "have always and deservedly been held in high esteem. Alkalies also are not without value in certain cases. . . . In my own experience, however," says Prof. Sands, "*local* treatment has generally proved the most efficient in the removal of gleet. Injections, mild or strong, superficial or deep; the occasional introduction of a full-sized bougie into the bladder. *When these fail*," he remarks, "the disease will often be found to depend upon *stricture of the urethra*."

I am quite willing to concede the influence of all the remedies and plans of treatment above enumerated. I recognize the fact that, whatever be the cause of a catarrh of the urethral mucous membrane, a condition of constitutional plethora, or, on the other hand, of extreme debility, would favor its continuance; and that irritating urine, such as would indicate the use of alkalies, copaiba, cubebs, etc., in any other trouble, would tend to palliate a gleet—nay, possibly, even cause

the cessation of the discharge, where it was kept up by the irritating quality of the urine. But it is a very well-known fact (and I am quite sure that it will not be disputed by my friend Prof. Sands) that *cessation* of the discharge does not mean *cure*. The reason, the chief, I believe, that has induced the professor to include the cure of gleet among the "unsolved problems of surgery" is, that, after cessation, under the varied treatment quoted, the discharge will, as a rule, return. A slight indiscretion in diet, a little vinous excess, a little venereal indulgence, of the most unexceptional character, *will bring back the gleet*.

The results of treatment, then, if they do not indicate the *cause* of gleet, teach us, at least, that it is not in any condition which such treatment can permanently control. Prof. Sands says, "When these and other similar methods of treatment fail, the disease will often be found to depend upon a stricture of the urethra."

Now, I would like to ask, in the most friendly and scientific spirit, why it is considered necessary to go through the above-mentioned category of constitutional remedies, and gleet specifics, and injections, and bougies, *before* this question is raised—nay, more, until it is *settled*? Again, I claim that, in order to discuss the subject of *gleet* intelligently, the subject of *urethral stricture* must *first* be considered.

I have stated it as my opinion that "chronic urethral discharges are, *as a rule*, dependent upon urethral strictures for their *continuance*," whether these strictures be the product of a gonorrhœal inflammation in the first instance, or the result of inflammation of other origin.

It is not likely that there will be any important disagreement as to the manner in which strictures are formed, but, I do not quite agree with Prof. Sands, nor with the authorities he quotes, in ascribing the *first* place in the causation of stricture to *gonorrhœa*. I recognize the fact that it is most often brought to our notice through the occurrence and persistence of this disease, and that all preëxisting strictures, or thickenings, or irritations, of the urethral mucous membrane, are increased and intensified by it.

I would like, for a moment, to call your attention to some



of the other—the *non-specific*—causes of urethral inflammation and stricture.

Sir Henry Thompson (whose views on so many points are in complete accord with those of Prof. Sands), in his work on "Stricture of the Urethra" (second English edition, page 114), headed "*Causes of Urethritis and thus of Permanent Stricture*," says: "Urine may possess an irritating quality from the predominance of an acid or an alkali in it; a persistence of either of these conditions must be recognized as one of the undoubted causes of organic stricture. Thus," he says, "Sir Benjamin Brodie states that alkaline urine is more likely to produce the disease (stricture) than that which is acid, and that persons secreting the triple phosphate are almost sure to have stricture sooner or later." Mr. Liston says, in reference to attacks of acidity of urine, that "their continuance, or frequent occurrence, may lay the foundation of disease of the urethra." And further, Sir Henry Thompson says (*ibid.*, page 115), "Excess of venery, protracted erections, and prolonged intercourse, are recognized causes of stricture." Lallemand, Ricord, Sir Everard Home, Acton, Gouley, Gross, and others, recognize *masturbation* as a cause of urethral stricture, and certainly if we can accept, with Sir Henry Thompson, excess of venery, etc., we cannot deny this influence to masturbation. I have myself seen several aggravated and undoubted cases which fully support this view; and, again, Sir Henry Thompson (*ibid.*, page 117) says, "The influence of gout and rheumatism are undoubted causes of spasmodic stricture—these diatheses, therefore, predispose in this manner to the accession of organic stricture."

Not to pursue the causation of urethral stricture further, for fear of wearying you, I desire now to ask your attention to a few observations upon, and natural deductions from, the foregoing citations from our most valued authorities.

In the first place, the influence of vitiated urinary secretions, excess of venery, prolonged erections, and protracted sexual intercourse, are distinctly recognized and insisted on, as a cause of organic urethral stricture, and this, too, by authorities whose facilities for urethral examination were most imperfect, and hence could only detect, positively, the

more advanced stages of stricture. It is but just, it seems to me, to infer that, in very many cases examined by them when symptoms of stricture were present, no stricture was detected. The method now pursued by Prof. Sands, Sir Henry Thompson, and many other less enlightened surgeons, would signally fail in detecting the earlier invasions of stricture in any urethra of a capacity above twenty-five millimetres in circumference.

Now, when we come to consider the proportion of men who, at some time in their lives, have suffered from acrid urinary secretions (from a gouty or rheumatic diathesis, and various other causes) from excessive venery, masturbation, etc., does it seem to you necessary to insist upon it that every subject of a gonorrhœa had a previously normal condition of his urethra?

Urethral stricture is recognized by Prof. Sands as *a cause* of gleet. What amount of contraction is, then, necessary to constitute a stricture capable of producing or prolonging a gleet? By the admirable casts of the urethra, which he has presented in his paper, he has, in four specimens, demonstrated a difference in the urethra of different individuals. These casts (carefully enlarged drawings of which I now present to you) will form an interesting basis for study in reference to what may be said to constitute a stricture.

In cast No. 1, the walls of the canal are seen to be smooth and quite free from indentations. No. 4 is almost equally so, except within an inch or so from the meatus, where two or three slight indentations are seen. No. 3 shows four or five wrinkles occurring at a point coincident, or nearly so, with the locality of the peno-scrotal angle during life, and correspond with the thickened folds of mucous membrane which are so commonly found at this point in examinations with the urethra-meter. No. 2 presents not less than *six distinct contractions* between the meatus and the bulb. It must be borne in mind that a force, of no insignificant character, has been used in the distention of the urethræ from which these casts were taken; and it may, I think, be reasonably presumed that any *accidental* wrinkles would have been straightened out; in short, that nothing but permanent organic contractions would have left their imprint upon the plaster cast.



FIG. 1.



Drawings of Urethral Casts presented by Prof. Sands on page 16 of his paper.  
Electrotyped from his wood-cut.

Whether these can be called strictures, or not, will depend very much upon what degree of contraction is considered worthy to be called stricture.

The practical point which this condition suggests, however, is that, whether we call these points strictures, or contractions, or wrinkles, they are certainly capable of interfering with the smooth and easy passage of urine; that they would furnish admirable points of lodgment for the solid constituents of the urine during an acid or an alkaline dyscrasia—very slight, it may be acknowledged, but very marked when compared with the smooth and regular outline of No. 1. Now, if we can suppose two urethræ, which shall be the counterparts of those from which casts No. 1 and No. 2 were taken, to be invaded by a gonorrhœal inflammation—which of them would, all other conditions being equal, escape with least damage—in which would a gonorrhœa be the least severe; which would be least likely to suffer with subsequent gleet? The urethræ which are represented by these casts were said to have been free from any evidences of disease: when, however, we recall the statement of *Foerster*, quoted by Prof. Sands, on page 7 of his paper, viz., that “blennorrhœa sometimes lasts a very long time without causing any material alteration of the urethral mucous membrane,” we may reasonably question the inference that Nos. 2 and 3 were free from disease, while we have ocular proof of the presence of conditions which would favor a contrary conclusion. If No. 2 can be accepted as representing a perfectly normal condition, the smooth and unwrinkled surface of No. 1 must then be acknowledged to vary from it in a very noticeable degree. Should it be claimed that the smoothness of No. 1 is accounted for by the less force used in making the injection, we will transfer the comparison to Nos. 3 and 4, which are sufficiently free from contractions to present a striking contrast with No. 2, although in case of these, as nearly as possible, the same force was applied.

Convinced, as I am, that *complete* freedom from obstruction in the muscular structure of the urethra is essential to the perfect performance of the act of micturition; that *complete* absence of points of friction is necessary to secure the greatest



immunity from local and reflex disease, I should no more feel justified in presenting cast or cut of urethra No. 2 as typifying a normal urethra, than I would present a neighboring orifice to you, as normal, when surrounded by the shriveled remains of half a dozen hæmorrhoids.

These casts were claimed to show, among other things, the incorrectness of my views, in regard to the absence, in a perfectly normal urethra, of that boat-shaped dilatation which is described by authors as occupying the first inch of the urethra, and termed the fossa navicularis. Four specimens are rather few to decide a disputed anatomical point, but, as far as they go, they prove the *correctness* of my position. It is entirely absent in No. 1. In No. 2, which is rich in dilatations and contractions, and with a meatus eleven millimetres smaller than any other part of the canal, it is present. In Nos. 3 and 4, if by courtesy it can be said to be present at all, it is within a quarter of an inch of a contracted meatus, and is merely a pouch-like dilatation, which I have always recognized as associated with a contracted meatus, and have never seen in connection with a meatus of the normal size.

In his discussion of this point, Prof. Sands states that I misquoted *Henle* in reproducing his representation of a frozen section ("Anatomie des Menschen," vol. ii., p. 424). It was the *illustration*, showing that the *normal* meatus corresponded in size with the urethra behind it, which I quoted—the tran-

FIG. 2.



Henle's Vertical Section of the Anterior Portion of the Penis.

script from a natural frozen section; *which proves my claim*—and *not* his subsequent reiteration of a conventional idea. He has *thrice* presented this plate as a transcript from a normal

condition of the penis at this point. The same drawing has been represented, by Drs. Van Buren and Keyes, as a normal condition under the title of "*Vertical Section through Glans and Fossa Navicularis.*"<sup>1</sup> It corresponds completely with my own numerous observations, on the living subject as well as on the cadaver. What Henle *says* is greatly weakened if not made wholly valueless, when his own transcripts from nature, as well as the careful observations of others, contradict his statements.

In speaking of examinations in the living subject, Prof. Sands says (p. 20), "In practice, we find, in the size of the meatus, a rough test for the calibre of the urethra." This has long been taught by authorities, and has been adopted by many surgeons as a guide for estimating the calibre of the deeper urethra. The idea may almost be said to be *pre-historic*, and possibly occurred to Adam on his first urination. If our great progenitor could have been examined with a bulbous sound, I doubt not that *his* meatus *would* have been found to correspond completely with the canal behind it, and hence, at that period (if man may then be believed to have been in physical perfection) the meatus would have proved an exact guide to the normal calibre of the urethra. When I now meet with such a case, I consider it the highest normal type of meatus. But, since then, indiscretions and other irregularities have crept into the world; and now, after six or more thousand years, the result is, that the meatus, among other things, has varied from its original type, so that, at the present day, if we accept the size of the meatus as a rough guide to the size of the urethra, we shall find it a very rough guide indeed. The fact is, that the meatus can be shown to be perfectly *inconstant* in its relations to the urethral calibre, and that in not more than one case in ten does it occur that the size of the meatus is a reliable test of the size of the urethra. In the examination of one hundred living subjects with the urethra-meter—

<sup>1</sup> "Genito-Urinary Diseases, with Syphilis," page 30.



1	was	18	mm. cir.	8	were	25½	mm. cir.
3	were	15	"	4	"	26	"
1	was	16	"	5	"	27	"
3	were	18	"	3	"	27½	"
2	"	17	"	2	"	28	"
3	"	19	"	1	was	28½	"
1	was	19½	"	5	were	29	"
3	were	20	"	3	"	30	"
2	"	20½	"	5	"	32	"
2	"	21	"	4	"	33	"
5	"	22	"	2	"	33½	"
1	was	23	"	2	"	34	"
1	"	23½	"	1	was	37½	"
7	were	24	"				
1	was	24½	"	100		24.72	
17	were	25	"				

Average size in one hundred cases, 24.72.

In no case was the urethra, in the one hundred cases, below a calibre of 26 millimetres—ranging from this to 39—the average being 32.95. I think, then, that we are forced to conclude that the size of the *meatus urinarius externus* is not in any sense or degree a guide to the urethral calibre.

It is worthy of remark that, in the one hundred examinations referred to, notwithstanding the very great disproportion between the size of the meatus and the calibre of the spongy urethra, no marked trouble on that account was noted. These were, however, cases which claimed to be free from inflammatory antecedents. It is probably the fact that, as long as the meatus escapes inflammatory action, it does not become a source of trouble on account of its diminutive proportions. We may have a meatus from the size of a mere pin-hole to the full size which corresponds with the calibre of the urethra behind it. None can, perhaps, be claimed to be *abnormal*, as long as the functions of the part are well performed; and hence, in the presence of so great variations, it might be difficult to fix upon the *highest normal type* of the meatus urinarius. We find, however, that various and grave difficulties and diseases are occasionally associated with a genito-urinary apparatus, where the meatus is not of the full size of the canal behind it, and that such difficulties are promptly relieved by

a surgical procedure which permanently enlarges the meatus to that size. The fact that such difficulties do not occur, when the meatus is of the full size of the canal immediately behind it, gives additional weight to the assumption. That condition, then, of these parts which insures the most complete functional integrity, and is least liable to become a source or seat of disease, and which is also least liable to induce, aggravate, or prolong disease in the contiguous parts, may, I think, be safely and appropriately accepted as representing the *highest normal type*. Now, by observation of the one hundred cases reported, the meatus will be found to correspond with the canal behind it, in *ten* cases, while *not one* exceeds this limit. Besides this, it can be most positively proved that contracted meatus prolongs and intensifies gonorrhœa, produces gleet, and is the source of varied and grave reflex irritations.

Profs. Van Buren and Keyes (p. 92) boldly state that "an individual with an average-sized penis, whose meatus will take only eight or nine (fourteen to fifteen f.), has *stricture* (congenital) of the meatus, although he never may suffer any inconvenience therefrom." Prof. Gouley (p. 103, "Diseases of the Urinary Organs") states that the proportion of strictures in this region, as compared to the entire number he has seen, is at least 30 per cent. Thus it stands: individuals *may* have a meatus, strictured more or less, and never suffer any inconvenience therefrom; again, this condition *may* give rise to grave trouble. Why this apparent difference? Simply, as I apprehend, that when the muscular structure of the meatus, and the urethra behind it, is in perfect condition, it is enabled to empty the urethra completely after urination. Let inflammatory action be set up in this locality, as may occur from extension of an infantile or an adult balanitis, or from gonorrhœa, or from any other cause, and a plastic exudation results, which, becoming organized, disables the urethral muscular structure, and it is no longer able to act efficiently in expelling the last drops of urine; they are retained, a dribbling results, and is the unvarying sign that such an accident has occurred. It is from this cause that the discharge from a gonorrhœa is retained, aggravating and prolonging the disease. Now, the only ra-

tional remedy, in this class of cases, is to relieve the obstruction; we cannot restore the disabled muscular structure, but we can relieve the obstruction, mechanically, by making the orifice to correspond with the size of the canal behind it, and thus enable the urethra to clear itself of its irritating secretions.

But Prof. Sands says: "The practice of slitting up the meatus is injurious and irrational," that "the normal meatus is narrow, and its size favors the projection of the stream of urine during micturition." It is not the *normal* meatus that requires any slitting, or any other operation; it is the division of the *abnormal* meatus—disabled through antecedent inflammatory action—which a *rational* treatment demands. I venture the assertion that thousands of cases of gleet exist to-day, which have been treated by copaiba, and constitutional remedies, and injections varied and frequent, and even by full-sized bougies, for years, and vainly, which this comparatively simple operation (of removing the obstacle to the complete emptying of the urethra) would promptly accomplish, besides affording immunity from recurrence, except from a fresh contagion.

But gleet, troublesome as it is, is by no means the only untoward result possible from a contracted meatus. This point is admitted by physiologists to be a sort of telegraphic depot for the whole genito-urinary system. Nor is it the genito-urinary system alone which may suffer from irritations of this locality. You have but to recall the fact, that a lascivious thought will cause a sensation at this point, and that slight irritation here will induce the sensuous thought. Depression of spirits, especially in youth; incontinence of urine; pain on ejaculation; neuralgias of the testicles, over the pubis, down the thighs, and even to the soles of the feet; spasmodic stricture, with or without retention of urine; prostatic irritation and enlargement; inflammation of the bladder and testicle—are each capable of being produced by this condition, in certain instances, as proved by the prompt disappearance of these troubles (often ineffectually treated by other and various means) through a free division of a contracted meatus. My paper on "Reflex Irritations throughout the Genito-Urinary

Tract," read before the New York Academy of Medicine in February, 1874, adduces no less than nineteen representative cases of this sort, with all the particulars of antecedent and subsequent conditions. In the London *Lancet* of January 29, 1876, Mr. Furneaux Jordan, F.R. C. S., Professor of Surgery, etc., of Birmingham, England, in speaking of the possible influence of a contracted meatus, writes thus: "I not unfrequently meet with the cause and its results. In boys a common result is cystitis, simulating stone in the bladder. Boys, however, often escape notable trouble; as men they are not let off so easily. With the cares, indigestion, gout (disguised or open), and other ailments, which increase the acidity of the urine, there come one or several of the results of stricture. One such effect is *urethritis*, which, by continuous extension, may lead to *prostatitis*, or *cystitis*, or *epididymitis*. There are some," he says, "who under such circumstances would affirm that the urethral inflammation had been caused by contact with some noxious fluid. . . . I will not," says Prof. Jordan, "here discuss the merits or demerits of a policy of uniform incredulity. My answer is this: often in cases of diminutive meatus, the bladder is affected first, then the prostatic urethra; then perhaps the inflammation may extend along the vas deferens, setting up consecutive orchitis, and from first to last there is no urethral discharge.<sup>1</sup> Frequency of *micturition*; *supra-pubic pain*; *mucus* or *blood* in the urine; are, singly or combined, the subject of complaint. . . . A *diminutive orifice* aggravates and prolongs a *gonorrhœa* or gleet or *stricture* and their ordinary *sequelæ*. The treatment," he says, "which I adopt for a small meatus is an incision—the result in all cases—a large number—*successful*. The success is not always rapid, especially in old-standing cases of cystitis; but, sooner or later, relief follows."

Sir Henry Thompson says:<sup>2</sup> "I have given complete relief to distressing symptoms of very long continuance, the cause of which was not suspected, by dividing an external meatus which, nevertheless, admitted a No. 6 English catheter;" and he cites three cases when the very simple operation necessary had given complete relief to symptoms "which had

<sup>1</sup> "Stricture of Urethra," second London edition, p. 249.    <sup>2</sup> *Ibid.*



long been regarded as of very obscure character." Now, if such troubles can be adduced as the possible effect of a contracted meatus, and such results can be shown by its division, can it be justly said that slitting a contracted meatus is *irrational*?

Prof. Sands (quoting from one of my papers on "Stricture of the Urethra") says: "If a urethra present, the normal calibre of which is equal to a circumference of 30 millimetres, and only a 29 bulbous sound will pass, without detecting obstruction, then the urethra is not 'about right.' It is strictured to the extent of one millimetre, and can never be a healthy urethra while that stricture remains." Then he says, "Let us inquire if these statements can be verified; if so, we shall find established an important principle in the treatment of gleet." The question of the measurement of the urethral calibre, which is involved in the statement quoted, is one of so great importance, that I shall not apologize for entering upon it with some degree of minuteness. As a mechanical proposition, there is no room to doubt but that, if the canal, that is, the ante-bulbous urethra, is 30, and 29 only will pass without detecting obstruction, obstruction certainly exists. This, however, as I apprehend, is not the point in dispute, but it is as to whether this minute obstruction, in the first instance, if present, can be made out, and in the second, if made out, can it prove a cause of trouble. The first point, then, to consider is, What do we understand by the normal calibre of the urethra? In order to settle this, and to meet the objections which have been urged against my own views on this subject, I will present briefly the method and results of urethral measurements by accepted authorities. From the year 1854 to 1875, Sir Henry Thompson taught that, "when 8 or 9 of the English scale could be passed easily through a given urethra, no stricture could be said to exist." In one of his recent lectures delivered at the University College of London, November 18, 1875,<sup>1</sup> he says: "Simply take a flexible English gum-elastic bougie, well curved toward the point, with a blunt end, not larger, as a rule, than 10 or 11 of our scale (that is, nineteen or twenty millimetres in circumference), and pass it very gently

<sup>1</sup> Reported in the London *Lancet*, December 11, 1875.

and slowly into the bladder. If it goes easily, above all, if it is drawn out without being held, and slides out with perfect facility, take my word for it he has no stricture, and *quoad* obstruction, wants no use of instruments whatever." It will thus be seen that Sir Henry Thompson fails to recognize the varied capacity of the urethra in different individuals, and practically reduces all urethræ to a common and fixed standard. It will also be observed that, within the last year, he has raised this standard from "8 or 9 English" (17 and 18 F.), to "10 or 11" (19 and 20 F.), that is to say, *about two millimetres*. Why he has done so does not appear.

Now, Sir Henry Thompson distinctly states, that "in the living body the walls of the passage are closely applied to each other in a state of inaction, so that the diameter is only calculable when distention occurs from some cause . . . . Indeed," he says, "the question of the diameter of the urethra must be considered as resolving itself, to a certain extent, into *the measure of its capacity of being extended*, and this is of greater practical importance than the mere width of the mucous membrane, when slit up after death;"<sup>1</sup> and yet Sir Henry fixes the urethral limit at 10 or 11 English, without the least reference to these facts.

Prof. Sands says that "we have properly the *normal* calibre of the urethra, *when it is moderately distended by urine during normal micturition*," and, although he remarks, "we cannot estimate this with accuracy, *I believe that it is not very large*." That is to say, it does not, in his estimation, make a calibre of more than twenty-five millimetres. He says, "*Finally, passing sounds exceeding twenty-five millimetres is very rarely necessary, either for the diagnosis or treatment of strictures of the urethra*." Prof. Sands thus virtually fixes the urethral calibre at *twenty-five millimetres*. Twenty-five millimetres is equal to 14 of the English scale. We are not informed why Sir Henry Thompson first fixed the urethral limit at "8 or 9," nor why he subsequently granted an extension to "10 or 11;" nor yet why Prof. Sands is willing to allow a calibre of 14. There is no evidence to show that these estimates are based upon any well-ascertained facts bearing

<sup>1</sup> "Stricture of the Urethra," Thompson, second London edition, p. 6.

upon this point. Profs. Van Buren and Keyes say (page 28 of their excellent work on venereal diseases,<sup>1</sup> and in italics), "*A fair, average, well-formed urethra measures about three-eighths of an inch in diameter;*" that is to say, thirty millimetres in circumference. The French school (as represented by Dr. T. B. Curtis, of Boston, in his essay which won the Civiale prize in 1873, and has thus the stamp of approval by the French Academy) says, "*The size of the human male adult urethra is seven millimetres in diameter,*" or 21 of the French scale.

The late Mr. Guthrie, so much appreciated as a surgical authority, both in Great Britain and America, says, "The urethra varies so much in different people, that it is scarcely worth inquiring into, particularly as the passage of instruments is always regulated by the size of the orifice;" . . . but, as to its *positive* size, he says: "I have a solid bougie which is rather more than half an inch (twelve and a half millimetres) in diameter. I had it made for one gentleman in particular, and it passed with perfect ease through the whole passage . . . Very few urethras," he further remarks, "will admit a sound of more than 12 to 16."

In view, then, of this apparent want of harmony (not to say definiteness), in arriving at a practical estimate of the normal urethral calibre, we must, I think, come to the conclusion that the authorities quoted must have taken *the size of the meatus, the volume of the stream, the results of post-mortem examinations, and the experiments on the extensibility of mucous membrane*, as a basis, and have struck a general average as to what ought, in their opinion, to constitute a normal urethral calibre. In summing up these independent, individual estimates, we find them as follows:

Sir Henry Thompson (10 to 11 E.) up to 19 or	20 millimetres.
The French school . . . . .	21 "
Prof. Sands up to . . . . .	25 "
Profs. Van Buren and Keyes . . . . .	30 "
Mr. Guthrie up to <i>over ½-inch diameter</i> . . . . .	about 40 "

Now, in a urethra of a calibre of 30, an instrument of 19 or 20 ("10 or 11" English) would pass a stricture of ten milli-

<sup>1</sup> "Genito-Urinary Diseases," etc., p. 28.

metres' value without discovering it; one of twenty-five millimetres would fail to appreciate a stricture of five millimetres' value or one-sixth of the entire calibre of the urethra; and should the normal calibre reach the size of 40, which it can be proved to do by Mr. Guthrie and myself, in rare cases, even an instrument of *thirty* millimetres in circumference would fail to detect a stricture involving *one-fourth* of the passage. It would, then, appear to be a matter of some importance, for a person suffering from symptoms of stricture, to ascertain the probable size of his own urethra before applying to a surgeon for aid; otherwise, he might apply to a disciple of the English school, who would not allow him a calibre of more than 19 or 20 ("10 or 11" English); or to a French surgeon, who would concede only 21; or to one who believes, with Prof. Sands, that "more than 25 is rarely necessary for the diagnosis or treatment of stricture;" for all these would certainly fail to detect, much less be able to appreciate, the extent of a stricture, above their estimates, in a urethra which should reach the fair average of the normal urethra of our more generous American authority, to say nothing of the possibilities of a urethra of the size of about forty millimetres in circumference, cited by that grand old English surgeon, the late Mr. Guthrie.

The conclusion is, then, forced upon us, that some method of arriving at an estimate of the normal urethral calibre must be adopted, which shall eliminate, as completely as possible, the element of individual opinion based upon generalities. The clear and practical view of Sir Henry Thompson, that "*the question of the diameter of the urethra must be considered as resolving itself into the measure of its capability of being distended,*" furnishes us with the only rational basis for a true appreciation of the urethral calibre in different individuals. Through a very great number of experiments, upon subjects living and dead, during a period of more than four years, the possibility of arriving at correct and uniform measurements of the urethral canal, by means of this instrument, the *urethra-meter* (which has already been described to you by Prof. Sands), has finally been demonstrated. By means of this it has been found possible to determine (and with scarcely more



discomfort than would result from the introduction of an ordinary sound or bougie) the *limit* of easy distention, and thus the *normal calibre* of urethræ, within one or two millimetres in almost every case. In a great proportion of one hundred cases, recently examined, this limit was defined *exactly*; and this

FIG. 3.

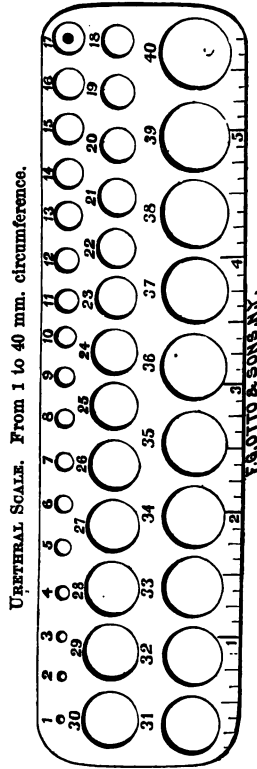
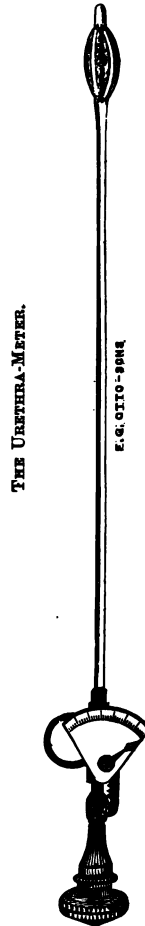


FIG. 4.



without regard to the contractions of the meatus, or the presence of strictures above 13 F., which is the size of the closed instrument. My examinations with the urethra-meter

have been, from the first, conducted with an entire knowledge and appreciation of the physiology and histology of the penis and urethra, as taught by authorities. It was fully recognized that the calibre of the urethra varied, anatomically, in different parts. The instrument was introduced, closed, to the bulbo-membranous junction, and then expanded slowly, until a feeling of *slight fullness* was experienced *by the patient*. If, *then*, it was *easily* and *painlessly* movable, it was drawn gently forward, and, if no positive obstruction was met, the urethra was considered free from stricture. If, however, it was arrested at any point, the instrument was turned down until it could pass, and the amount of obstruction was noted from the dial. If the holding was slight, and at a point of usual anatomical narrowing, it was not considered important, unless the instrument was distinctly resisted on being pushed back at such point.

After making a great number of examinations, I was led to appreciate an important difference in the calibre of different urethræ, and that an average standard was impossible. That while thirty millimetres was the full measure of one man's urethra, that of another would as freely admit a No. 40. solid sound through its entire length, and into the bladder.

Another point, and one which has attracted some, but not sufficient, attention, was that of the proportionate relation, which I came to observe, between the size of the urethra and the penis with which it was associated. After an extended experience on this point, I am prepared to state that this relation is constant, and is about 1 to  $3\frac{1}{2}$ ; in a penis of three inches circumference the urethra would be 30,  $3\frac{1}{4}$  32,  $3\frac{1}{2}$  34,  $3\frac{3}{4}$  36, 4 38,  $4\frac{1}{4}$  to  $4\frac{1}{2}$  40; and that an estimate of calibre made on this basis is a valuable guide when the urethra-meter is not available. It must, however, be borne in mind that the circumstances under which examinations are made occasionally (though seldom) vary, and that some experience is necessary in order to recognize and appreciate the conditions which temporarily affect these relations.

Even late authorities state that a large penis may be associated with a small urethra, and that a small penis may

accommodate a large urethra. This important statement will be proved untrue by the results of my examinations.

Out of the one hundred cases presented in the annexed tables,<sup>1</sup> the size of the urethra corresponded with the size of the penis, exactly in accordance with my claim, in thirty-nine cases :

				89 cases.	
Deviating from it	1	millimetre,	36	"	
" "	2	millimetres,	17	"	
" "	3	"	2	"	
" "	4	"	1	"	
" "	5	"	3	"	
" "	6	"	1	"	
" "	7	"	1	"	
				100	"

On page 21 of his paper, Prof. Sands relates *his* experiments with the urethra-meter. I am not surprised that, from his experience in *twenty* cases, he should arrive at conclusions on some points somewhat at variance with my own. The urethra-meter is an instrument which, like the *stethoscope*, requires a familiarity with its use, for which no anatomical knowledge, or dexterity in the use of other instruments, can fully compensate. The tactile skill which is required to appreciate the least amount of distention which urethral mucous membrane will bear, without damage, and yet shall give the assurance of its full expansion, will bear comparison with the appreciation of the *true respiratory murmur* in a chest-examination. Prof. Sands did find, however, that the instrument showed variations, in different localities of the urethra, corresponding with those which he had previously demonstrated on the dead subject. He says, "If the above facts can be verified, they prove indubitably that the assumption of an unvarying calibre for any urethra is unwarrantable; and it is plain that such an assumption must lead to the gravest errors in practice."

Now, I, for one, am sure that "the above facts" *can be verified*, and I most cordially agree with Prof. Sands in his statement, as to the error of considering the urethra of unvarying calibre, as well as in regard to probable consequences of such an

<sup>1</sup> The tables referred to were necessarily omitted for want of room.—ED.



error. This is *the* error which is practically made by those who estimate the calibre of the urethra by the size of a bougie, and *not* by any one who makes an intelligent use of the urethra-meter. Prof. Sands has misapprehended me when he infers that I am accustomed to take the calibre of the bulbous portion of the urethra as a *measure* of what all parts of the urethra in front of this portion "ought to be."

The passage quoted from my article on gleet, etc., from which this conclusion is drawn, is as follows: "At this point (the bulbo-membranous junction) the bulbous portion of the instrument (the urethra-meter) is to be expanded by means of a screw at the handle, until a *feeling of fullness* is experienced, when, if there is no stricture at the point of trial, the hand on the dial-plate will indicate, *with sufficient certainty*, the normal calibre of the urethra under examination."<sup>1</sup> The *feeling of fullness* spoken of, referred, in my mind, to the *sensation* of the *patient*; and this I found was experienced, as a rule, before the true capacity of the canal, at that point, was reached; from the extreme sensitiveness which exists in some cases, the sensation of the patient affords no reliable guide in ascertaining the calibre of the ante-bulbous portion, with the urethra-meter. This abnormal sensitiveness is rarely present at the bulb, and thus the instrument, raised to a point occasioning a feeling of fullness to the patient (and not one of arrest to the operator) indicated, "with sufficient certainty," the calibre of the ante-bulbous urethra, and not the size of the bulbous urethra, which authorities state, and I then fully believed was, as a rule, much larger.

My meaning was perhaps not as clearly expressed as it should have been, but the errors which might arise from the impression that the bulbous and ante-bulbous portions are of the same size are perhaps not so great as Prof. Sands intimates, or as I myself would have premised, before making my recent urethral measurements of one hundred cases of supposed normal urethræ.

In these, the measured difference between the bulbous urethra and the part anterior to it was—

<sup>1</sup> "On Gleet and its Relations to Urethral Stricture, American Clinical Lectures," p. 253, by F. N. Otis, M. D.

In	35	cases	1	millimetre. <sup>1</sup>
"	21	"	2	millimetres.
"	18	"	3	"
"	6	"	4	"
"	2	"	5	"
"	2	"	6	"
"	2	"	7	"
"	1	"	11	"
"	13	"		no difference.

The average difference in the one hundred cases was  $2\frac{5}{100}$  millimetres, and the calibre of the antebulbous portion averaged 32.95.

In my previous report of one hundred cases, in a paper read before the State Medical Society in February, 1875, and which were examined with the view to detecting stricture, and not to ascertain the normal calibre, the average calibre was  $31\frac{1}{2}$ . The difference of about two millimetres in the average of the first and second hundred cases may, I think, be accounted for by the more rigid, thorough, and methodical carrying out of the plan of measurement in the more recent examinations. In this connection, as opposed to the traditional idea, it will be interesting to quote the opinion of that eminent English surgeon, the late Mr. Guthrie, who says:

"This bulbous portion of the urethra is said to be larger than the anterior part, but I do not believe that it is, *although it may appear so.*"

Perfect security against mistaking a normal narrowing for stricture may always be had, by examining *from before backward*. If the canal anterior to the contraction is of distinctly larger calibre, this localized contraction must be accepted as a stricture. I recognize the elements of doubt, as to the cause and nature of localized urethral contractions, in some cases, especially as *post-mortem* examinations often do not show any lesion of the mucous membrane over a point where stricture has been recognized during life. Various conditions, resulting from persistent irritation of mucous membrane, may obtain, which are capable of causing changes—possibly atrophy, with contraction of the trabecular structure of the corpus spongiosum, or obliteration of its meshes, and

<sup>1</sup> 1 millimetre equals  $\frac{1}{25}$  of an inch.

which might escape the observation of those who were looking only for cicatricial deposits. One thing is certain, that the subject has not yet received, from our microscopical experts, the attention its importance demands. The practical fact, however, remains, that whatever permanently constricts a localized portion of any urethra is practically a stricture, and capable of causing the effects of stricture, and is also amenable to the same method of treatment.

The value of the examinations of one hundred cases, repeatedly referred to during the course of this paper, will be better appreciated by a knowledge of the circumstances under which they were made. The subjects of examination were, some in my own wards in Charity Hospital; others, through the courtesy of my colleagues, Drs. Keyes, Howe, Piffard, and Frankel, were selected from their wards. Quite a large proportion, fully one-half, were patients in Bellevue Hospital, kindly placed at my disposal by my friends Profs. Sands, Stephen Smith, and Dr. Erskine Mason.

The examinations were conducted by me, in the presence and with the assistance, on different occasions, of Drs. Stephen Smith, George A. Peters, F. J. Bumstead, H. G. Piffard, L. Bolton Bangs, W. T. Bull, and various members of the house-staff of Charity and Bellevue Hospitals. In the accompanying tables the names of each of the gentlemen, as far as possible, are associated with the cases examined or reviewed by them.

In three of the cases, a reëxamination was made after death; two cases, in the presence and with the assistance of Dr. Stephen Smith, Dr. A. Jacobi, and Dr. L. Bolton Bangs; and the third in the presence and with the assistance of Dr. Freeman J. Bumstead, Dr. George A. Peters, and Dr. Bolton Bangs. In the first two the reëxamination was found to accord completely with that made during life; in the third, the distensibility of the bulbous urethra was increased four millimetres; but the measurements in the anterior portion of the canal and size of the meatus remained the same. The measurement of the flaccid penis, in each case, was less by one-quarter of an inch than during life; but as, in the former, the measurement was made after the removal of the integument, it so far shows that the measurement of the



flaccid penis during life does not differ greatly from a *post-mortem* measurement.

The results of examination were carefully noted by my friend and associate Dr. L. Bolton Bangs, whose sole office it was to record them. The tabulation, which is appended, was also made by him, solely, and has been subjected to no revision by any other person.

In regard to the case of fourteen strictures (reported by me to one of our medical societies, and subsequently published in the NEW YORK MEDICAL JOURNAL of April, 1874) referred to by Prof. Sands, I desire to protest against this grave accident to my patient being brought forward to discredit the results of my method of examining the urethra, especially so, as this warrants the inference that I am in the habit of discovering and operating upon strictures that do not exist. I am aware of the claim of Sir Henry Thompson, that rarely more than three or four strictures occur in a single urethra. Pursuing the same general mode of examination, it is not difficult to appreciate the incredulity of Prof. Sands in regard to the existence of fourteen strictures in a single urethra. If a man thrust his hand into a fire, there will be no dispute but that he may have, resulting, as many scars as he has received burns. In the same way there can be no limit to the number of urethral scars, which become strictures, except by limiting the degree and continuance of the gonorrhœal, or other fire, which has inflicted the primary injury.

This drawing, which was presented, in company with the living subject, before the New York Medical Journal Association early in 1874, is a fairly correct diagram of the number, size, and locality of the fourteen strictures. They were made out by me, on several occasions, before the operation, and at the time of the operation these measurements were rehearsed and confirmed, under æther, by Dr. George A. Peters and Dr. Deforest Woodruff, who assisted me during the operation. The strictures were found, in a penis of three inches, to vary from twenty-two millimetres to one-third of a millimetre, and extended to  $6\frac{1}{2}$  inches, beyond which the urethra was practically impermeable. The perineal section was performed for the posterior strictures, and dilating urethrotomy for those

anterior. The strictures were, with the exception of three bands deep in the perineal urethra, made out with the bul-

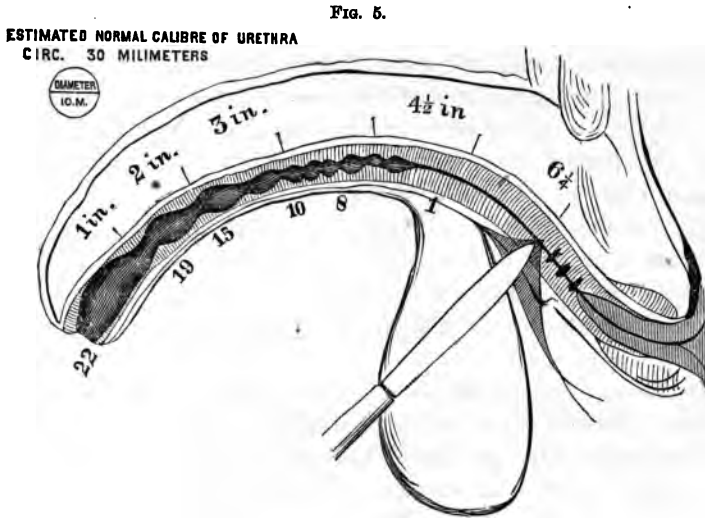


Diagram showing locality of the fourteen strictures in the case of W. C. H.

bous sounds; the latter were recognized in the passage of the Maisonneuve blade, by me, and distinctly appreciated by Drs. Peters and Woodruff.

I was more than gratified to learn, from so able a surgeon as Prof. Sands; from one who so thoroughly enjoys the confidence of the medical profession and of this community and country, that he thought so well of the operation of internal urethrotomy. "Some of the most gratifying results in modern surgical practice," says Prof. Sands, "have been achieved by this method. But," he continues, "I believe it applicable chiefly to the treatment of close strictures, and as an auxiliary to dilatation." I could have wished that he had accorded to this operation of internal urethrotomy, so highly commended, a broader scope. Prof. Sands announces himself as "a firm believer in gradual dilatation." For my own part, I can only consider gradual dilatation of stricture, except so far as it may be necessary to prepare the way for urethrotomy, in the light of a temporary expedient, and would use it, only as I would temporize with a vesical calculus, with demulcents and seda-

tives, when the condition of the patient was such as to forbid the use of the scalpel or the lithotrite. I fully recognize the responsibility of so pronounced an opinion on this important matter, and I trust that, during the discussion which is to follow, some sound reasons will be adduced to show why *urethrotomy* should be confined to grave and close strictures; why a resilient urethral obstruction should be made the subject of oft-repeated stretchings—never without risk, and perhaps for a lifetime—instead of the prompt, rational, and what appears to me the more surgical treatment by *division*. Is it his fault, or that of his surgeon, that the subject of a gleet is so often made to pay a wearying tribute to one member of our profession after another, until at last he drops into the clutches of that class which Sir Henry Thompson so graphically describes as hanging on the outskirts of our honorable profession: who will extort his last dollar in exchange for a placebo. I believe it can be proved, that every gleet is the result of stricture, and that it is a true and safe economy to search it out in its inception—to divide it, and thus promptly restore the urethral calibre to its integrity, and before the damage it may occasion, has implicated tissues and organs to an extent which may imperil life. Every stricture is a mortgage bearing compound interest, and the wise man will promptly pay it off. Every gleet is a call for payment. You may for the time, with syringe and bougie, drive off this implacable, persistent dun, but he will return, in one guise or another, until the debt is paid, or the property is forfeit.







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